MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Admiral MANNERS, President, in the Chair.

Dr. F. Brünnow, Astronomer Royal for Ireland, Dublin;
Rev. Robert Crowe, Huddersfield;
Thos. Cooke, Esq., Southampton Street and York;
John Knox Laughton, Esq., Royal Naval College, Portsmouth; and
William Locke Lancaster, Esq., Down's Park Road, Hackney,

were balloted for and duly elected Fellows of the Society.

Note on an Aurora Borealis, April 2nd, 1869.
By John J. Plummer, Assistant at the Durham Observatory.

A short time since the idea occurred to me that something might be added to our knowledge of the nature of the Aurora Borealis, by means of the analysis of its light by the spectroscope, and I resolved to examine its spectrum upon the first opportunity. A very fine Aurora was visible at Durham, soon after midnight on April 2nd. It is the result of the observations made upon that occasion, that I have now the honour to communicate to the Society.

The light of the Aurora appeared to the eye as white, perhaps slightly bluish, and not of the more common ruddy hue; there were few or no rays, but broad sheets or waves of light succeeded each other rapidly, proceeding from the arch to a height of about 30° from the horizon. When the light was examined by the spectroscope, one *bright* line was immediately seen. At the brightest parts of the arch, this line was perhaps equal in intensity to that of the most conspicuous of the lines in the Nebula of *Orion*, it presented ordinarily a somewhat hazy appearance, but at times would flash out sharply and well defined. It was difficult to make any accurate measurement of its position, but two approximate mea-

sures were taken, by illuminating the field suddenly, by a hand-lamp and placing the cross-wires as nearly as possible on the position in which the line had been seen. These two estimations are respectively $19^{rev.}\cdot40$ and $19^{rev.}\cdot25$.

The following are the readings of the principal Solar lines, as measured on March 6th, 1869, for referring these numbers to their places in the spectrum:—

rev.	rev.	rev.	rev.	rev.
A = 14·909	*a* = 15·397	B = 15·934	C = 16·496	D = 18·000
B = 19·961	*b* = 20·357	F = 21·751	G = 25·307	H = 28·503

The two conspicuous pairs of lines in the air spectrum near D, would be represented on this scale by the numbers $17^{rev.}\cdot89$ and $18^{rev.}\cdot61$; but I cannot believe that the two aforesaid estimations are so much in error that the latter of these lines is the one seen in the spectrum of the Aurora. I am, therefore, led to the conclusion that the spectrum of the Aurora is *not* identical with that of air; but as some doubt may exist on this point, I shall anxiously await the next apparition of an Aurora to verify this result. The advanced season, however, renders it unlikely that an opportunity will occur for some months. I have, therefore, thought it right to communicate my present results, however imperfect they may be.

It may be worthy of remark that the line in the Aurora spectrum agrees closely with that of the more conspicuous of the lines in the spectrum of *α Orionis* between the solar lines D and E. I have measured this line upon two occasions, Feb. 12 and Feb. 13, and the resulting measures were $19^{rev.}\cdot350$ and $19^{rev.}\cdot381$ respectively. There is also a tolerably conspicuous line in *Aldebaran*, near the same place. It may be also necessary to state that all these measurements have been referred directly to the sodium line D, by the observation of the spectrum of the metal from the salt with an alcohol flame, both before and after the observations. They are thus strictly comparable.

Durham Observatory,
April 6th, 1869.

Addendum.

Since the above paper was written, I have been informed by Mr. Huggins, that a similar observation has been made by Angström, and confirmed by Struve, during the winter 1867–68. I was totally unaware of this at the time of communicating the above paper. I will add that I have also had an opportunity of confirming my own observation. An aurora of surpassing magnificence was visible in the North of England upon May 13 ; a corona was formed, and every part of the sky for a short time was filled with streamers. Some of these, in the West and South-east especially, were of a deep red colour, and the single line in

the spectrum was seen here precisely as in the white parts of the aurora. I succeeded in making one good estimation of the position of the line from the red streamers, sufficient to identify it with that of the other measures made later on the white arch which was formed in the North-west after the corona and streamers had partially or wholly disappeared. This estimation gave $19^{rev.}\cdot18$ as the position of the line. The subsequent measures give mean results of $19^{rev.}\cdot03$ and $19^{rev.}\cdot10$ respectively, which are believed to be tolerably close approximations, though not quite so satisfactory as had been hoped for.

No other lines were seen.

The extreme tenuity of the matter forming the aurora was demonstrated by the fact that Winnecke's Comet was visible with the $6\frac{1}{2}$-inch refractor of the Observatory, through one of the densest streamers, without any other inconvenience than that of the brightness of the field of view.

Durham Observatory, May 17th, 1869.

On the Determination of Longitudes by Electric Telegraph. By E. D. Ashe, Commander R.N., Director of the Observatory, Quebec.

As I have had some experience in determining the longitude by electric telegraph, and, as it is likely to be useful to several of our Members in different parts of the world, I send an account of fixing the position "Rivière du Loup."

Sir William Logan (Director of the Geological Survey) requested me to obtain the latitude and longitude of "Rivière du Loup," which is about 130 miles below Quebec.

I started by railway on the 4th February; took with me a 24-inch transit; and a sidereal chronometer.

On my arrival, I called on the telegraph operator, who I found could not read by sound, and, consequently, I resolved to fix my transit-instrument in his garden, so that I should have only to take the chronometer into his office when I was ready to send signals.

I generally select a boulder, or get a large stone, upon which I place the instrument; erect a hut to keep the wind off; lead the telegraph wire into it; and, with a little portable telegraph-instrument that I always carry with me, I have all that is required when I am ready to send signals.

In this instance all the Earth was covered with snow for several feet in depth, and nothing like a stone for the support could be obtained; so I got a flour-barrel, removed the snow from the earth, and placed the barrel on the ground; then filled it with sand, poured two or three buckets of water over it and around it, and, as it was freezing, placed a square piece of board on the top; and as the thermometer was several degrees below

zero, in a few minutes the whole was one solid mass. I then with boards built a hut.

I had to wait several days for a clear night, when I got the instrument into the meridian. As the country was covered with snow, and as all the fences are rails, you are sure of getting a "meridian mark" in some of the many posts. I had one about four miles off, a black line upon a white ground.

I have frequently been annoyed by boys, particularly in Upper Canada, where they are of the English type, who, when they see a light, commence throwing stones, and it is impossible to count seconds under those circumstances. On this occasion I heard one night a boy's voice, saying, "Please, sir, will you let any body in?" "No! but you may look through the crack." I cannot say if he took advantage of my kind offer; but if he did, he was as quiet as a mouse.

I got both latitude and longitude in the first four clear nights. After I had sent "time" to Quebec, I removed the transit-instrument, and drew a circle through the three points made by the three supports of the instrument, and moved one of the points 90° on the circle, and then I had the instrument on the prime vertical for latitude.

I was detained 19 days at the hotel waiting for clear weather. The expenses for travelling, building hut, paying operators at both ends of the line, amounted to 13*l.* This does not include remuneration for myself.

Quebec sent "taps" from the "mean time clock," commencing at the even minute, and continuing beating seconds until the 50 second in each minute, and this they did for six minutes. "Rivière du Loup" noted the "coincident tap" with the sidereal chronometer, marking the second and the series; and thus we had the difference of longitude to the hundredth of a second at once.

"Rivière du Loup" then sent "sidereal seconds" to Quebec, and this was repeated twice from each end of the line.

I cannot help thinking that this plan is the easiest and least troublesome, only requiring the telegraph line for half an hour, which is of great consequence.

The Latitude by Transit in Prime Vertical.

	°	′	″
α Aurigæ ..	47	49	44
1071 B.A.C. ..	47	49	45
			89
Latitude of Rivière du Loup	47	49	44·5

At Quebec 11 stars were observed for clock error,—7 stars, lamp west, and 4 stars, lamp east; deviation obtained by *Polaris* and ε *Ursæ Minoris*, and level frequently applied.

At Rivière du Loup 13 stars were observed,—7 lamp east and 6 lamp west. Deviation by *Polaris* and ε *Leporis;* and level frequently applied.

1st Set of Signals from Quebec.

	h	m	s
Coincident tap sent by Q. at ..	8	52	10·00
Clock fast			−45·37
Mean Time at Q.	8	51	24·63
Ditto at R. du Loup	8	58	3·37
Diff. Long.	0	6	38·74
Coincident tap noted at R. du Loup	5	18	35·00
Clock slow		+1	1·71
Sid. Time	5	19	36·71
Ditto at Mean Noon	20	20	5·04
Ditto since Mean Noon	8	59	31·67
Retardation		1	28·30
	8	58	3·37

1st Set from Rivière du Loup.

	h	m	s
Coincident tap noted by Q. at	9	28	5·00
Clock fast			−45·37
Mean Time at Q.	9	27	19·63
Ditto at R. du Loup	9	33	58·38
Diff. Long. 2d Set		6	38·75
,, 1st Set		6	38·74
Coincident tap sent by R. du Loup	5	54	36·00
Clock slow		+1	1·71
Sid. Time at R. du Loup ..	5	55	37·71
Ditto at Mean Noon	20	20	5·04
	9	35	32·67
Retardation		1	34·29
	9	33	58·38

The next two sets of signals were turned into sidereal time, giving —

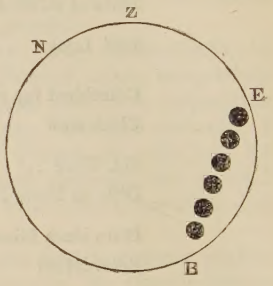
	h	m	s
A Mean Difference of Longitude of	0	6	38·70
And the Grand Mean of the 4 Sets was	0	6	38·725
Longitude of Quebec	4	44	49·02
Longitude of Rivière du Loup ..	4	38	10·295

Observatory, Quebec, March 4, 1869.

On the Transit of Mercury over the Sun's Disk on 5th Nov. 1868, *as seen at Vizagapatam.*

(*In a Letter from A. V. Nursing Row, of Vizagapatam, to Prof. C. Piazzi Smyth, Edinburgh.*)

The annexed diagram represents the disk of the Sun at the time of the Transit, as visible to an observer at Vizagapatam, for direct image.



Z represents the vertex, or the highest point of the Sun's disk.

N the north point.

B the point of first contact.

E the point of last contact.

	Mean Time. h m s
First contact	10 58 39
Last contact	2 35 57

The dark spots in the diagram represent the dark disk of *Mercury* when crossing over the Sun's disk.

"I have the pleasure to enclose herein a copy of a printed notice to friends in this neighbourhood, containing a rough diagram of the Transit of *Mercury* as it was seen here in Vizagapatam. The weather certainly favoured us on the 5th of November, and we enjoyed well the observation of the Transit. The planet was seen as an intensely dark spot on the Sun's disk. When the planet approached half-way of the transit, some of my European friends and myself observed a wavy tint of light darting from the upper edge, disturbed at times, but continued until the planet had passed some distance from the highest point of the line of transit.

"Suspecting it to be an effect of disturbed focus, I often refocussed the instrument and changed the eye-pieces, besides other precautions, yet we always observed the same phenomenon, but with high powers we found the edge of the planet was not so intense as in the centre, and especially so at the line of light in question. The telescope we used was the one which I explained in my pamphlet on the recent solar eclipse, and shows the stars without wings, &c., therefore I am very anxious to be enlightened on the cause of such appearance, if it were not in any way an optical delusion.

"A scientific friend who was then present remarked to me that he was informed somewhere that the planet, when leaving the Sun's disk, would have the appearance of a flask. I tried my best and watched for it, but did not find with my instrument anything but a dent on the edge of the Sun's disk at the last contact."

Note by C. P. S.—The writer of the above extract is a Hindoo gentleman of Vizagapatam, whose family has been much given to science through two generations. He possesses an extensive Observatory, both astronomical and meteorological, and is now about adding to the former a 6-inch Equatoreal, with driving-clock, micrometer, and spectroscope; and to his library all the purchasable volumes of the *Monthly Notices of the Royal Astronomical Society*.

The telescope with which the above observations were made has an object-glass of 4·8-inches aperture, by the late W. S. Jones, of London, and was fitted up in Mr. Nursing Row's own workshop on a large and original kind of altazimuth stand, supplied with vernier-circles, levels, and slow-motion screw-movements. In his solar-eclipse pamphlet of last August, Mr. Row thus alludes to the telescope:—

"As to the character of the object-glass of the present telescope I may add that it gives no colour around the image of any object; and that the image of any fixed star it shows is free from the optical appendages of wig or wings. It can be judged from this that the present telescope will discharge its duties faithfully."

The failure of Mr. Row to see the irradiation-distorted figure of the planet *Mercury* is probably due to his appearing, from his description, to have expected to see it, not at the internal, but at the external contact of Sun and planet. What, however, the *wavy* phenomenon was which he saw so persistently during the middle of the transit I do not pretend to suggest any explanation of.

On a Method of Imitating the Transit of an Inferior Planet.

By H. W. Hollis, Esq.

At the close of the November Meeting of the Royal Astronomical Society, I mentioned to the President an experiment which I had made with a view to obtaining artificially the appearances presented immediately after the first or before the last internal contact at the transit of an inferior planet. I was led to make this experiment from having observed, at the transit of *Mercury* on the 5th November last, the formation of the black connecting band or ligament between the planet and the Sun's limb, immediately before the last internal contact. On that occasion I found it quite impossible, on account of the formation of such apparent connecting band, to say within several seconds when the real internal contact took place.

At the top of a tower 150 feet high, I placed a projecting spar of wood, and from the outer end of this spar I suspended by a fine wire a lead ball, half an inch in diameter. I then went to a position upon the ground, 286 yards distant from the ball, which, from that position, would subtend an angle of about 10 seconds, or nearly the apparent diameter of *Mercury* at transit.

I waited until, by the effect of the Earth's rotation, the ball was seen projected upon the Sun's disk, and upon then observing the ball with a 6-inch refractor, and power 220, I obtained, at its passing off the disk, an exact representation of the formation of the black connecting ligament. Of course, the duration of this artificial transit was very short, and the apparent motion of the ball across the disk, or rather, of the disk behind the ball, was such as to render the approach of the ball to the limb infinitely more rapid than the corresponding appearance in the real phenomenon, and the ligament was only visible for a second or two, but still it was sufficiently distinct to render the experiment an interesting one.

On mentioning this experiment to the President, I was requested by him to give a written description of it, to be laid before a subsequent Meeting; and I now do this the more willingly from having observed that the possibility of making such an imitation of a transit was referred to at the last Meeting.

Keele, Newcastle, Staffordshire,
5th May, 1869.

Note on the Preparations desirable for Photographic Observations of Phenomena such as Transits of Venus. By Major Tennant.

Mr. De La Rue has in the *Monthly Notices* for December last again brought forward the claims of Photography to be used in permanently recording for examination at leisure the phenomena of the Transits of *Venus* in 1874 and 1882. I believe the probable value of this method is very great, though I feel that evidence has still to be adduced on this subject before it can be unreservedly trusted; and I propose here offering some suggestions as to apparatus and procedure, in the hope of inducing a fair trial.

First. It appears to me that an effort should be made to get rid of instrumental distortion; for I think the necessity for investigation of its amount and of applying corrections a serious drawback. If a reflecting telescope with *unsilvered* glass mirrors were used, the light would be very much diminished; and still more so if the image were optically enlarged. In a Newtonian telescope this might be done by either a convex or concave lens achromatised for the actinic rays, of which arrangement the latter would make the apparatus more compact; or the telescope might be a Cassegrain, thus becoming still more compact and manageable. An instantaneous shutter would in any of these cases allow a fairly large aperture to be used, and this having its centre part removed would give good definition.

Next. I would make a modification in the way of releasing the shutter. At Kew it is held against a spring by a thread which is cut with scissors. I would propose that it be retained by an electromagnet, and that the current forming this should also pass through

a chronograph. If an observer at a separate telescope had a break circuit key he could at any moment photographically record a phenomenon he saw and the instant of its occurrence. An assistant at the heliograph would be needed to replace the shutter and insert the dark slides, unless the observations are far apart. By using a repeating slide and combining the measurements on one plate, very valuable normal relations of the Sun and planet would be found, and observations might be made during the ingress or egress (possibly two of the black drop) in considerable numbers.

There is an advantage in this mode of observing the transit of *Venus* to which Mr. De La Rue has not alluded.* If accurate micrometrical observations can be made by means of photographic pictures, then the range of suitable stations can be greatly enlarged; for any two stations, 140° or 150° apart, can have the observations combined by choosing a suitable time, and, of course, by means of equations of condition, the observations of stations in all sorts of places could be used with their proper weight. All the photographs could be measured by the same micrometer, whose errors of all sorts could be very carefully examined.

Before, however, it could be expected that reliance should be placed on these photographic results, it is clearly necessary that a rigorous examination should be instituted of the forms of apparatus &c. An image formed, as that in Mr. De La Rue's apparatus at Cranford, or mine at Guntoor, is evidently almost free from distortion, save those arising from the collodion film. It is small, and would admit of measurement to great accuracy with a micrometer-screw of fine pitch. If enlarged by any of the means before described, coarser means of measurement would produce results equally accurate, but the distortion of the enlarging lens would be involved. Lastly, the small images might be enlarged by a specially made lens (as in the case of the Guntoor photographs), and these might be measured. In some respects this method would be better than the last, as the original images might be better centred in the axis of the enlarging lens, which, too, would be of longer focus.

All these methods must, to produce confidence, be tried. Probably as convenient a method as any would be to take photographs in various ways of any nearly total eclipse, and to treat them rigorously as micrometrical measures would be treated in an observatory, deducing the probable errors of one observation in each way.

I believe (for my own part) that the results would prove photograph registration to be thoroughly reliable as a substitute for

* Mr. De La Rue has pointed out the facility with which the nearest approach can be got from the photographs at any one station, but if photographs be taken at two stations at a time when *Venus* is in the plane which includes them both as well as the Earth's centre, these will show the whole effect of parallax; and it is the positions at these instants, and not the nearest approaches to the Sun's centre, which should be compared.

micrometrical measurements in the ordinary way, and possibly they might be much better. Even if the suitability were disproved, it would be of some value and might save a waste of energy during the transit of *Venus*.

If it be determined to send parties to photograph during the next total eclipse in the south of India, then a Photoheliograph might be placed at the centre as well as at either edge of the shadow-path as well as at the apparatus for the total phase only, and a very complete examination of the question at issue would be possible.

Mooltan, March 22, 1869.

Comments on Major Tennant's Note on the Preparations for Photographing the Transits of Venus. By Warren De La Rue, Esq.

I beg leave to offer a few remarks on some points in Major Tennant's communication.

1st. With respect to the choice of instruments; a reflecting telescope with unsilvered mirrors would certainly present the advantage over chemically achromatised refractors, in which, as in the Kew Photoheliograph, the image is enlarged by a secondary lens, of giving an image free from optical distortion; that is to say, the Cassegrainian and Gregorian present these advantages, but not so the Newtonian, if an achromatised lens is to be used to enlarge the image before it falls on the sensitive plate. My experience has proved to me that the necessity for obtaining a flat field so complicates the problem that hitherto no lens has been made which answers the conditions to be solved.

Now with respect to the Cassegrainian and Gregorian forms of instrument; it is of course necessary to make a provision for the free transmission and dispersion of that portion of the light which passes through the mirrors; they would have necessarily to be supported in cells open at the back, and their back surfaces would be advantageously made concave (convex towards the first or reflecting surface), in order to prevent any light being reflected by the back of the mirrors and forming a secondary image. The heat emerging from the back of the principal mirror would seriously interfere with successful photographic manipulations, and, although it is conceivable that special provisions could be made to diminish this inconvenience by reflecting aside the transmitted rays, it would complicate the apparatus.

By employing a Newtonian without an enlarging lens the image would be considerably smaller, but it would be free from optical distortion, and by the use of a Herschelian prism the light would in this case be reduced by a second reflection from glass (as in the other forms of reflectors), but the operator would be clear of any of the refracted heat rays, and would work at ease. I give, therefore, the preference to the Newtonian.

I do not think, however, that the reflector is on the whole so convenient an instrument for observing transits of *Venus* as a refractor on the Kew model; for the optical distortion of such refracting instruments can be ascertained practically and allowed for, without in any way interfering with the final correctness of the deduced results.* Moreover, strictly speaking, the reflector is not absolutely free from optical distortion on account of the photographic plate having a flat and not a curved surface.

2nd. As regards the measuring instrument I am afraid that I have failed to convey a correct idea of the capabilities of the micrometer I devised for measuring the eclipse-pictures of 1860, and which I have described and figured in the *Phil. Trans.* of 1862, pages 373–380. Major Tennant evidently thinks that the instrument is only calculated to deal with quantities no smaller than the $\frac{1}{1000}$th to the $\frac{1}{2000}$th of an inch, representing when used in conjunction with the Kew Photoheliograph $0''{\cdot}5$ to $0''{\cdot}25$; and that the substitution of a micrometer screw for the scale of equal parts and Vernier would be an improvement. The instrument is really provided with a micrometer screw, which may be used for making the measurements of any required minuteness independently of the scale of equal parts, which was added for the special work in hand, namely, for the measurement of pictures already enlarged by the secondary magnifier to four inches in diameter. The clamping apparatus for throwing the micrometer screw in and out of action is also a special provision for the measurements of the actual Sun pictures, for it would be exceeding fatiguing to give several hundred rotations to the screw whenever a measurement had to be made. There really is no limit to dimensions of the instrument, which is adapted for the measurement of any rectangular or other co-ordinates or any angular quantities which may be desired, and by increasing the optical power of the microscope it may be used for measuring astronomical photographs with any degree of precision for the special requirements of the case. It would be quite unnecessary, and indeed detrimental, to enlarge photographs for micrometrical measurement when once they have been procured by any form of heliograph.

3rdly. With respect to the localities to be selected, the employment of the photographic method of observing transits of *Venus* has, as I have stated in the *Monthly Notices*, vol. xxix. No. 2, page 49, the advantage of rendering us independent of conditions favourable or indeed essential for eye-observations; for a few photographs obtained at each station would afford the means of ascertaining the path of the planet and its position at any given moment; and hence the proposal to determine the position of the planet's centre in relation to the Sun's centre really includes the particular case contemplated by Major Tennant in the foot-note appended to his paper; but it is possible that the Sun might be obscured at the critical time, at one of the two stations selected.

* *Monthly Notices*, vol. xxix., p. 51.

I did not describe the method of ascertaining the position-angle of the planet (at p. 51 of *The Monthly Notices*, vol. xxix.) because the means of determining the position-angle of wires in the foci of the primary and secondary lenses of the Kew Photoheliograph has been fully described in the *Phil. Trans.* for 1862. I may mention, however, that it can be ascertained within 1′ of arc.

On the Solar Eclipse of 1871. By Major Tennant.

(*Extract from a Letter to Mr. De La Rue.*)

I sent last mail to Mr. Airy a proposition to superintend three parties in the south of India in the Eclipse of 1871. I fear he will think it far too extravagant for recommendation. The idea of three parties is I believe yours; I was sorry there were not three last August, but in that stage of the matter it was hopeless, and the point I lay most stress on is that each party who go to photograph the obscuration should have a Photoheliograph to make use of, and to try rigorously by purely astronomical tests. A small probable error for the measures from a picture would be conclusive, unless it was shown that all would have a common error. Now if two pictures could be taken on one glass, these *could not have* any common error. If the pictures were taken in the principal focus of the telescope there would be no distortion except a very minute amount arising from equal parts of the image not corresponding to equal angles. This could be corrected (if needed at all) by mere theory.

I have not added in the proposition what is I think important, and that is, an examination of the Corona Spectrum with a large telescope, and arrangements to show whether it has any lines. The cost of this would be a good deal. The telescope *would not need equatoreal mounting* if an assistant were available, but a spare assistant would probably be equally costly, and he would need, as in M. Janssen's apparatus, a good large telescope himself. I should, too, like to be further on with other things before looking at this. I should not undertake it (if I had three parties) till I were sure of the leaders at all events.

I wish I could give some estimate, but that I cannot. Using chronometers and leaving out clock &c., would save, but I should prefer the other plan, and I do not look to it in any sense as all money spent on the Eclipse, for a large part would clearly be money spent in explanatory experiment as to the applicability of Photography in 1871 to delicate astronomical purposes.

If three Photoheliographs at stations near each other, whose absolute and relative place are well known, give accordant values for the tabular errors of all sorts, and assign a small probable error to one observation or photograph, then I think the matter will be settled beyond dispute, and I believe, if proper care be taken, the result will be decisive.

I cannot be at home to start it, and must hope that some of

my friends will urge the matter. You have yourself taken so much interest in the application of Photography that I know you will aid if you approve of the means. If it is to be carried out by *me* I should be got to England as soon as possible, and I certainly say I shall have no objection to an early start. Three months, however, would probably elapse between the Secretary of State making up his mind and my reaching England, and that is a very sensible portion of what is left for preparations.

On the Solar Eclipse of August, 1869.

By R. T. Paine, Esq.

(*Extract from a letter to the Secretaries of the Society.*)

You are aware that on the 7th of next August the shadow of the Moon will pass over North America, and that, on that day, in some of our *Atlantic* States there will be a total eclipse of the Sun. It will be the fourth within ninety-one years, and the third in this century, and there will be only one more before 1901 (May 28th, 1900), unless we include Texas in the *Atlantic* States, over which the shadow will pass in the fourth return of our great eclipse of June 16, 1806, on July 29th, 1878.

As such great interest has of late years been taken in these phenomena, and as the eclipse of next August is the first return of the one so successfully observed by many English astronomers, in July, 1851, in Sweden, it has seemed to me as very probable that some of them would visit North America for the purpose of observing also the one now approaching ; hoping that this opinion may prove correct, I take the liberty herewith to transmit a computation of the path of the centre of the Moon's shadow, and to make the request that you would deliver it to any one who may contemplate undertaking this voyage, and to whom it may be acceptable. This computation was made by myself some months ago, for my own information and has not been made public. It was made from the longitude, latitude, and other elements of the Sun and Moon, as very carefully deduced from the English *Nautical Almanac,* except that for Encke's Solar Parallax on August 7th, the quantity $8''{\cdot}83$ was used.

The duration of totality when longest (near Mount St. Elias) will be quite four minutes, but it gradually diminishes to $3\frac{1}{2}$ ($3^m\ 28^s{\cdot}4$) at our boundary line, latitude 49° ; at the City of Des Moines, the capital of Iowa, it will be exactly three minutes ; at Springfield, the capital of Illinois, $2^m\ 51^s$, and about 30 miles S.W. of Raleigh, North Carolina, $2^m\ 27^s$. The territories of Montana and Dakota are we believe, like British America, on the central line, thinly inhabited, but Fort Union on the Missouri (which is reached by steamboats from St. Louis) appears by the map to be very near that line, and, therefore, if correctly laid down, is favourably situated, also in Iowa, the cities of Des Moines, Boonsboro', and Burlington, are all under or very near

that line ; and although distant 1400 or 1500 miles from us, are reached in three days, on continuous railroads from this city and New York, whilst Macomb and Springfield in Illinois, New Albany, in Indiana, and Louisville in Kentucky, equally well situated, and at the distance of about 1000 miles, can be reached in two days, and Abingdon, in Virginia, and Raleigh, in North Carolina, in one day from Washington City, from which they are distant less than 400 miles by railroad.

Now that we are told it is not impossible to see the red flames around the Sun, even when it is not eclipsed, it seems strange they have not always been seen when the obscuration by the Moon was complete. Indeed, the first good account of them I had from an actual observer, was only 18 years ago, from my lamented friend, the late Prof. G. P. Bond, who saw them in Sweden during the totality of July, 1851. I believe I may say they were not noticed by the late Dr. Bowditch, who observed with great care the eclipse of 1806, which occurred here, on a beautiful day in June, with a totality of five minutes, only half an hour from noon, and certainly I did not perceive them in the total obscuration of November 30, 1834, at Beaufort, in South Carolina, where it was central. The duration of totality was to be sure only little more than a third of that here in 1806 ($1^m\ 49^s{\cdot}6$). The second contact I closely watched through my telescope with a power of 40, and a double (red and green) screen, but saw no flames ; as soon as possible I read off, and recorded with a lantern the time by a chronometer, and then with an opera-glass looked at the corona and saw that magnificent and appalling spectacle, and at the same time the star *Antares*, that day in conjunction with the Sun, and therefore distant only by its latitude, $4\frac{1}{3}$ degrees. The sky was very clear throughout the eclipse, and the edges of the Sun and Moon sharply defined, and without any apparent distortion whatever.

So much was said last year in the magazines and newspapers about the length of totality ($6^m\ 48^s$) where the eclipse of August 18 was central at noon, which was called "unequalled in centuries past and to come," that merely from curiosity I was led to look into the subject, and the result of my examination thereof was published in a communication in one of our papers, a copy of which I take this opportunity to transmit to you. Since last August I have carefully recomputed, by the tables of Carlini and Burckhardt, but using the Greenwich semi-diameter of the Sun, and Hansen's of the Moon, the second and third contacts of the eclipse of August 7, 1850, for latitude 17° 50′ N. longitude 141° 49′·4, W. where it was central on the meridian, and found they took place as follows :

	G.M.T.	L.M.T.	
	h m s	h m s	
Second contact	9 22 55·3	11 55 37·7	A.M
Apparent Conj. of ☉ and ☽ in Longitude	0 26 50·9	0 59 3·3	
Third contact	0 29 46·7	0 2 29·1	P.M.

	Moon's Ap. Long. — Sun.	Moon's Ap. Lat. — Sun.	Moon's Ap. S.D. — Sun.
Second Contact	−72″·98	+3″·60	+73″·07
Ap. Conj. of ☉ and ☽ in Long. ..	0·00	+0·23	
Third contact	+73·01	−3·14	+73·08

Altitude of Sun and Moon at meridian 88½°; duration of totality, 6m 51s·4, or 3½s longer (if I am correct) than in August 1868. The matter is, however, one merely of curiosity, and may, therefore, be considered as of little importance. I think, however, we may say that in one or two former returns of the series of eclipses to which that of last August belongs, the duration of totality, where central at noon, was quite as long as it was then.

The rapid increase of interest in astronomical phenomena is highly gratifying; perhaps there is no stronger evidence of the rapidity of this increase than that afforded by the degree of attention paid to several of the last, and to the last returns, of the above series of eclipses, which is remarkable, indeed, for the length of totality, where these eclipses are central. The returns of June 20, 1778 (central in United States), and those of July 1796 and 1814, took place, it is believed, with little if any notice; and although at the return of July 27, 1832, only thirty-six years ago, the Moon's shadow passed at about 8 A.M., over two of those quite *near* islands, the Bahamas, viz. Great Inagua and Grand Turk (the latter in N. latitude about 21½°, W. longitude 71½°), and caused a totality of quite five minutes, it did so, it is supposed, without a single scientific observer. In February of the preceding year I saw in the south-eastern part of this State, the first of the four *ring* eclipses I have witnessed, and was thereby rendered desirous of beholding a total obscuration, but the want of a direct communication induced me to forego that pleasure at that time: probably the same difficulty was experienced by those European astronomers who wished to go to Africa or the Bahamas for the same purpose, and the difficulty was then considered insuperable, but it would have easily been overcome by a small part of the zeal and enthusiasm displayed by the observers in India in August, 1868.

Path of the Centre of the Moon's Shadow over the Earth, Saturday, August 17, 1869, *according to Elements of the Sun and Moon as given by the Tables of Le Verrier and Hansen in the English Nautical Almanac, except that for Encke's Parallax of the Sun on August* 17, 8″·83 *was used. Ellipticity of the Earth* $\frac{1}{300}$*th.*

	G.M.T. h m s	L.M.T. h m s	Latitude. ° ′	Longitude. ° ′
Central Eclipse begins at	8 46 8·6 P.M. and	4 35 56·6 A.M.	52 43·7 N.	242 33·0 W.
,, ends	11 15 48·5	6 45 54·5 P.M.	31 17·2	67 28·5
Greatest N. Lat. of C. E.	9 18 30	9 40 14·0 A.M.	64 15·7	174 34·0
Eclipse central at Noon	9 45 59·7	0 5 26·1 noon	61 49·4	145 8·4
Duration of Central Eclipse	2 29 39·9.			

Eclipse Central at

Siberia.

G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude. ° ′
8 46 8·6	4 35·94 A.M.	52 43·7 N.	242 33·0 W.
47 0	5 13·19	55 29·3	233 27·1
48 0	32·25	56 45·2	228 56·3
50 0	59·95	58 24·3	222 30·8
52 0	6 22·31	59 34·6	217 25·4
54 0	41·97	60 29·6	213 0·5
56 0	7 0·04	61 14·3	208 59·4
58 0	16·98	51·5	205 15·3
9 0 0	33·09	62 22·7	201 43·6
2 0	48·54	48·8	198 21·9
4 0	8 3·45	63 10·7	195 8·2
6 0	17·90	28·7	192 1·5
8 0	31·94	43·4	189 0·9
10 0	45·62	55·0	186 5·7
12 0	58·97	64 3·7	183 15·5
14 0	9 11·99	10·0	180 30·1
16 0	24·72	13·9	177 49·2
18 0	37·17	15·6	175 12·5
20 0	49·34	13·1	172 39·9
22 0	10 1·25	12·8	170 11·3

North America.

9 24 0	10 12·89	64 8·7	167 46·6
26 0	24·28	2·8	165 25·8
28 0	35·42	63 55·3	163 8·7
30 0	46·31	46·3	160 55·3
32 0	56·97	35·9	158 45·4
34 0	11 7·41	24·2	156 38·9
36 0	17·61	11·1	154 35·9
38 0	27·59	62 56·9	152 36·1
40 0	37·36	41·6	150 39·6
42 0	46·92	25·2	148 46·2
44 0	56·29 A.M.	7·7	146 55·7
46 0	0 5·46 P.M.	61 49·3	145 8·1
48 0	14·45	30·0	143 23·3
50 0	23·25	9·9	141 41·2
52 0	31·89	60 48·9	140 1·7
54 0	40·36	27·1	138 24·6
56 0	48·68	4·5	136 49·8
58 0	56·85	59 41·2	135 17·3
10 0 0	1 4·88	17·2	133 46·8

G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude W ° ′
10 2 0	2 12·78	58 52·5	132 18·3
4 0	20·55	27·2	130 51·7
6 0	28·21	1·2	129 26·9
8 0	35·75	57 34·7	128 3·8
10 0	43·18	7·5	126 42·3
12 0	50·52	56 39·7	125 22·2
14 9	57·77	14·4	124 3·4
16 0	2 4·95	55 42·4	122 45·8
18 0	12·06	12·9	121 29·1
20 0	19·11	54 42·9	120 13·4
22 0	26·10	12·3	118 58·5
24 0	33·04	53 41·1	117 44·4
26 0	39·93	9·3	116 31·0
28 0	46·79	52 36·9	115 18·1
30 0	53·63	4·0	114 5·5
32 0	3 0·46	51 30·4	112 53·1
34 0	7·28	50 56·2	111 40·8
36 0	14·12	21·4	110 28·2
38 0	20·98	49 46·0	109 15·3
40 0	27·87	9·7	108 1·9
40 30	29·60	0·4	107 43·5

United States: Territory of Montana.

10 41 0	3 31·33 P.M.	48 51·3	107 25·1
42 0	34·79	32·7	106 48·1
43 0	38·28	13·8	10·8
44 0	41·78	47 54·8	105 33·3
45 0	45·30	35·5	104 55·5
46 0	48·83	16·0	17·5
47 0	52·40	46 56·3	103 39·0
48 0	55·99	36·4	0·1
49 0	3 59·61	16·2	102 20·8
50 0	4 3·27	45 55·7	101 41·0
51 0	6·96	35·0	0·6

Territory of Dakota.

10 52 0	4 10·69	45 13·9	100 19·7
53 0	14·45	44 52·5	99 38·2
54 0	18·27	30·8	98 56·0
55 0	22·13	8·7	13·0
56 0	26·06	43 46·3	97 29·1
57 0	30·05	23·5	96 44·3

State of Iowa.

G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude W. ° ′
10 58 0	4 34·11 P.M.	43 0·1	95 58·4
59 0	38·25	42 36·3	11·3
11 0 0	42·47	12·0	94 22·9
0 30	44·63	41 59·7	93 58·1
1 0	46·80	47·2	33·0
30	49·01	34·6	7·4
2 0	51·24	21·8	92 41·4
30	53·51	8·8	14·9
3 0	55·81	40 55·6	91 47·9

Illinois.

11 3 30	4 58·13	40 42·3	91 20·5
4 0	5 0·50	28·7	90 52·5
30	2·91	14·9	23·9
5 0	5·35	1·0	89 54·7
30	7·85	39 46·8	24·7
6 0	10·40	32·3	88 54·0
30	13·01	17·5	22·4
7 0	15·67	2·4	87 49·9

Indiana.

11 7 30	5 18·39	38 47·1	87 16·6
8 0	21·19	31·3	86 42·1
30	24·08	15·1	6·3

Kentucky.

11 9 0	5 27·05	37 58·6	85 29·2
30	30·13	41·6	84 50·6
10 0	33·31	24·0	10·4
30	36·61	5·8	83 28·3

S.W. Corner of Virginia.

11 11 0	5 40·06	36 46·9	82 44·1
15	41·84	37·2	21·1

North Carolina.

11 11 30	5 43·68	36 27·3	81 57·3
45	45·56	17·1	32·8
12 0	47·51	6·6	7·4

G.M.T. h m s	L.M.T. h m	Latitude N. ° ′	Longitude W. ° ′
11 12 15	5 49·51	35 55·9	80 41·1
30	51·57	44·9	13·9
45	53·72	33·6	79 45·5
13 0	55·95	21·8	15·8
15	58·27	9·6	78 44·7
30	6 0·70	34 56·9	12·0
45	3·26	43·6	77 37·3
14 0	5·99	29·5	0·2

Atlantic Ocean.

11 14 15	6 8·90	34 14·6	76 20·2
30	12·05	33 58·6	75 36·7
45	15·51	41·3	74 48·6
15 0	19·40	21·9	73 54·0
15	23·93	32 59·7	72 49·8
30	29·67	32·3	71 27·5
42	36·39	0·5	69 49·6
45	39·00	31 48·5	11·3
48·0	43·43	28·3	68 5·5
11 15 48·5	6 45·91	31 17·2	67 28·5

The Time of Beginning and End, &c. of the Eclipse of August 7, 1869, at four places where it will be Total.

United States, Boundary.

Lat. 49° 0′ 0″; Long. 107° 42′ 48″.

	Greenwich M.T. h m s	Boundary M.T. h m s
Beginning of the Eclipse	9 31 3·4	2 20 12·2 P.M.
,, Totality	10 38 47·1	3 27 55·9
Apparent Conj. of ⊙ and ☽ in Longitude	10 40 31·6	3 29 40·4
End of Totality	42 15·7	31 24·5
,, the Eclipse	11 44 39·4	4 33 48·2
Duration of Totality	..	3 28·6
,, Eclipse	..	2 13 36·0
At Ap. Conj. Diff. App. Latitudes ☽ − ⊙	..	☽ −0″·15
At Beg. and End of Totality, ☽'s D. − ⊙'s D. = ☽ + 50″·79 and 50″·68		
Point of Beginning, Direct Vision, from ⊙'s Vertex to the right hand		104°

City of Des Moines, Iowa.

Lat. 41° 50′ 24″; Long. 93° 39′ 18″.

	Greenwich M.T.	Des Moines M.T.
	h m s	h m s
Beginning of the Eclipse	9 56 47·0	3 42 9·8 P.M.
,, Totality	10 59 23·0	4 44 45·8
Apparent Conj. of ⊙ and ☽ in Longitude	11 0 53·2	46 16·0
End of Totality	2 23·0	47 45·8
,, the Eclipse	59 32·5	5 44 55·3
Duration of Totality	..	3 0·0
,, Eclipse	..	2 2 45·5
At Ap. Conj. Diff. App. Latitudes ☽ − ⊙	..	☽ − 0″·03
At Beg. and End of Totality, ☽'s D. − ⊙'s D. =	☽ + 47″·73 and 47″·60	
Point of Beginning, Direct Vision, from ⊙'s Vertex to the right hand		120½°

Springfield, Illinois.

Lat. 39° 49′ 0″; Long. 89° 30′ 0″.

	Greenwich M.T.	Springfield M.T.
	h m s	h m s
Beginning of the Eclipse	10 3 10·4	4 5 10·4 P.M.
,, Totality	11 3 59·7	5 5 59·7
Apparent Conj. of ⊙ and ☽ in Longitude	5 25·2	7 25·2
End of Totality	6 50·5	8 50·5
,, the Eclipse	12 2 25·3	6 4 25·3
Duration of Totality	..	2 50·8
,, Eclipse	..	1 59 14·9
At Ap. Conj. Diff. App. Latitudes ☽ − ⊙	..	☽ − 0″·7
At Beg. and End of Totality, ☽'s D. − ⊙'s D. =	☽ + 46″·62 and 46″·49	
Point of Beginning, Direct Vision, from ⊙'s Vertex to the right hand		124°

Raleigh, North Carolina.

Lat. 35° 46′ 48″; Long. 78° 38′ 19″.

	Greenwich M.T.	Raleigh M.T.
	h m s	h m s
Beginning of the Eclipse	10 14 46·5	5 0 13·2 P.M.
,, Totality	11 11 20·2	56 46·9
Apparent Conj. of ⊙ and ☽ in Longitude	12 22·0	57 48·7
End of Totality	13 23·2	58 49·9
,, the Eclipse	12 5 27·6	6 50 54·3
Duration of Totality	..	2 3·0
,, Eclipse	..	1 50 41·1
At Ap. Conj. Diff. App. Latitudes ☽ − ⊙	..	☽ − 24″·34
At Beg. and End of Totality, ☽'s D. − ⊙'s D. =	☽ + 43″·57 and 43″·46	
Point of Beginning, Direct Vision, from ⊙'s Vertex to the right hand		130°

The geographical position of Raleigh is supposed to be well known, but the assumed latitude of Des Moines and Springfield may be too great. A diminution of 10′ therein would make the computed times of the four contacts *later* at the former by 17·2, 19·2, 15·7, and 16·0 seconds, reduce the length of totality 3·5 seconds, and make the Ap. Lat. of the Moon—Sun's at conjunction + 5″·88, and at Springfield later by 16·8, 16·9, 15·4, and 14·0 seconds, reduce the length of totality to $2^m\,49^s\cdot3$, and increase the least distance of centres, Moon North, to 5″·70.

The width of the Moon's shadow in Iowa, Illinois, Indiana, &c., will be about 100 miles.

On the Corona round the Sun in Total Eclipes.

By Jos. Baxendell, Esq.

(*Extract from a Letter to Mr. Huggins.*)

I have been much interested in reading the report of a discussion which took place at the last Meeting of the Royal Astronomical Society, respecting the corona which is seen round the Sun during total eclipses. Recent observations and discoveries indicate very clearly that this corona is not an appendage of the Sun. What then is it? or how is its appearance to be accounted for? In a conversation I had some weeks ago with Professor Roscoe on the subject, I reminded him that in a paper which I read to the Literary and Philosophical Society in March, 1864, giving the results of a discussion of an immense number of magnetical and temperature observations made in different parts of the world, I showed that these results could be best explained by assuming the existence of an irregular nebulous ring circulating about the Sun, nearly in the plane of the ecliptic, and at a mean distance of 0·169; and I suggested to him that the reflexion of the Sun's light from the matter of this ring might be the real cause of the appearance of the corona in total solar eclipses. The view taken by M. Faye and the Astronomer Royal appears to me to be quite inadmissible. During that portion of the time of totality, when the corona is seen to the greatest advantage, no part of the Earth's atmosphere within a considerable angular distance of the Sun and Moon receives any direct sunlight, and, therefore, none can be reflected from it. Nor can I conceive how the corona can be due to any atmosphere about the Moon that could not be detected under other circumstances than those of a total solar eclipse. The differences in the appearances of the corona as seen and described by different observers cannot fairly be adduced as an argument against its cosmical origin; on the contrary, they are precisely what might be expected to occur with such an object seen under slightly different conditions of transparency of the atmosphere, and with eyes of different degrees of susceptibility to faint impressions of light.

Further investigations which I have made since I communi-

cated the paper referred to above to the Literary and Philosophical Society have yielded no results that at all militate against the conclusions at which I had arrived.

Cheetham Hill, Manchester,
May 6, 1869.

On the Determination of the direction of the Meridian with a Russian Diagonal Transit Instrument. By Captain A. R. Clarke, R.E. (Abstract.)

The paper contains an account of the determination of the direction of the meridian at Findlay Seat, a mountain in Elginshire, 1104 feet high, 1868, Oct. 13 to 31, by means of a diagonal transit instrument,—that is, one in which the rays of light (instead of passing straight from the object-glass to the eye-piece) are bent at right angles by a prism in the central cube and so pass out at one of the pivots. The instrument used was made in Russia by M. Brauer, in the workshops of the Imperial Observatory at Pulkowa, and was lent by the Astronomer Royal to the Director of the Ordnance Survey; focal length of telescope about 31 inches, and aperture $2\frac{5}{8}$ inches; distance between the uprights 19 inches. In a copy of the instrument made by Messrs. Troughton and Simms, the telescope has 32 inches focal length, and $2\frac{6}{8}$ inches aperture. The general conclusion is, that considering the inherent difficulty of determining the direction of the meridian in so high a latitude, and the circumstances under which the observations were made,—in stormy weather, on the top of a high mountain, until the party was fairly driven away by snow and the destruction of some of the tents—the results furnish strong evidence in favour of the stability and excellence of the instrument. The observations and calculations were made in connexion with the Ordnance Survey, but were, by the permission of Col. Sir Henry James, presented to the Society. The observations at Findlay Seat were made by Quarter-master Steel and Sergeant Buckle, Royal Engineers.

Account of a series of Drawings of Mars.
By John Joynson, Esq.

The Opposition of *Mars*, which occurred on the 13th February last, has not been a favourable one for observations of the appearances on his disk; and, in addition to this, observers have had to contend with most unpropitious weather, the moist, open winter rendering our own atmosphere almost entirely unfit for the noting of the markings presented when the planet could be seen.

The accompanying drawings are selected from ninety, which

I have been able to make at various times. They represent the planet during a complete rotation, and are intended as a sequel to those sent to the Society for the three previous Oppositions. They are so arranged that each diagram on the several sheets represents the same aspect of the planet, the appearance being, of course, altered somewhat at each opposition by the change in apparent position of the axis of the planet.

I have endeavoured, through the Society, to interest others in obtaining similar sets; but so far I am sorry to say that I have not met with much apparent success. It will be readily seen that there has been no absolute change in the Band, and the Channel from it to the North Pole, since the first set of diagrams were made in 1862. It is true that the set now sent appears to differ from that first taken, and it would be difficult at first sight to say that they were views of the same aspect of the planet; but those for 1864 and 1867 will sufficiently prove that to be the case, as the gradual alteration can by their aid be clearly seen. I have no doubt but this will continue during the future oppositions, and that it will be found that the same appearances will return in due course.

It has been observed at each opposition that the power of the eye-piece used, and also the presence or absence of hazy cloud, materially affected the colour of the disk; and I am disposed to think that the reddish look the planet sometimes had was chiefly, if not entirely, caused by our own atmosphere, as at these times even the reddest parts looked brownish, and sometimes yellow.

The colour of the Band and Channel has been dark, without any of the greenish appearance noted in former oppositions.

Waterloo, near Liverpool,
April 7, 1869.

Remarks on Mr. Joynson's Paper of Occultations.

By John J. Plummer, Esq., Observer at the Durham Observatory.

In the *Monthly Notices*, vol. xxix. No. 5, are some observations of Occultations by Mr. Joynson, observed near Liverpool, accompanied by some remarks, the object of which seems to be, to express a doubt as to the accuracy of the approximate times of these phenomena, given in the *Nautical Almanac*. Believing that the error was most probably on the part of Mr. Joynson, I computed for the three stars, *Aldebaran*, 119 *Tauri*, and 120 *Tauri*, the times of their disappearance and reappearance at Greenwich, with sufficient approximation; and it is perhaps superfluous to add, with results absolutely identical with those of the *Nautical Almanac*. I also computed by the same formulæ the times of the occultations of these stars for the latitude and longitude of Mr. Joynson's Observatory. The following are the results:—

1869.			Calc. G.M.T. for Greenwich.	Angle from North Point.	Calc. G.M.T. for Liverpool.	Angle from North Point.	Obsd G.M.T. at Liverpool.
			h m	°	h m	°	h m s
Jan. 23	Aldebaran	Dis.	8 48·7	97	8 44·6	105	8 44 16
		Reap.	10 4·7	289	10 0·0	282	10 0 0
24	119 Tauri	Dis.	8 14·5	143	8 18·0	155	8 17 26
		Reap.	9 9·5	237	9 0·2	225	9 0 45
24	120 Tauri	Dis.	8 47·8	117	8 46·7	126	8 46 27
		Reap.	9 59·6	260	9 53·1	252	9 48 13

It will be seen that the agreement between the observed and computed times for Liverpool is sufficiently exact, with the exception of that for the last star, the observed time for the reappearance of which appears to be 5 minutes in error. The effect of the difference of parallax upon the observed time of the occultation of a star by the Moon, evidently, cannot be obtained without systematic calculation.

Permit me to call the attention of those astronomers who observe occultations at places remote from Greenwich, to a paper by the Rev. T. Chevallier, published in vol. xix. of the *Memoirs* of the Royal Astronomical Society, by which the effect of parallax on an occultation is readily calculable. I have observed during one year above twenty occultations, the approximate times of which have always been computed beforehand by tables founded upon that method. This computation is the work of less than two minutes, and the mean error in the approximate time thus obtained, I have found to be 34 seconds, a sufficient degree of accuracy, since the times in the *Nautical Almanac*, and upon which the computation depends, are given only to the nearest minute. What is perhaps of more importance, especially for observations of reappearances at the bright limb, is, that the approximate angle from the north point of the Moon is obtained with a mean error of $\frac{1}{10}$ of a degree only. Those who have computed the corrections due for the effect of parallax at different places by the long and rigorous formula, will appreciate the value of this short cut; and I shall be happy to compute tables upon the same plan as those I have myself used, for any Observatory where their value may be appreciated.

Durham Observatory, April 16th, 1869.

On the Period of η Argûs. By John Tebbutt, Jun.

(*Extract from a Letter to Sir J. F. W. Herschel, Bart.*)

"I have read with much interest the paper by Mr. Abbott on *η Argûs* and its surrounding nebula, at page 200 of the 28th

vol. of the R. A. S. *Monthly Notices*, and your own remarks at page 225 of the same volume. You are already, doubtless, aware that I have carefully watched the variations in the light of this star during the past fifteen years. As it is not improbable that the star may soon begin to increase in lustre, I have watched it for the past twelve months, and am still watching it, with more than ordinary interest, in order to secure, if possible, the time and magnitude of the minimum. I have, therefore, on several occasions since April last carefully compared *η Argûs* with the principal stars in the Catalogue on page 42 of your 'Results of Astronomical Observations at the Cape of Good Hope from 1834–8.' In order to be assured of the identity of the comparison stars with the principal ones in your Catalogue, I determined their positions approximately by means of a telescope of 48 inches focal length and $3\frac{1}{4}$ inches aperture, roughly mounted as an equatoreal, a transit telescope of 2·1 inches aperture mounted on Y's secured to substantial piers of brick and stone, and a half-seconds chronometer. The comparisons with the telescope of $3\frac{1}{4}$ inches aperture were made with a ring-micrometer, whose diameter = 21′ 3″·6. The following Table gives the individual results of the comparisons, *η Argûs* being in each case employed as the comparison star:—

"*Co-ordinates with reference to η Argûs of certain Stars (Mags. $6\frac{1}{2}$ to 7) in the Catalogue on page 42 of Sir J. Herschel's 'Results of Astronomical Observations at the Cape of Good Hope*, 1834–8.'

Date of Comps.	No. 1 or L. R.A.	No. 1 or L. Decl.	No. 2 or X. R.A.	No. 2 or X. Decl.	No. 522 or U. R.A.	No. 522 or U. Decl.
1868.	m s	′ ″	m s	′ ″	m s	′ ″
May 19	...	...	...	...	0 37·85 W.	18 24·9 S.
21	3 45·36 W.	17 29·9 S.	3 41·35 W.	0 12·3 N.	0 37·98 W.	18 30·4 S.
	...	...	...	...	0 38·86 W.	18 37·3 S.
	...	...	...	...	0 39·11 W.	18 35·6 S.
	...	...	...	...	0 38·61 W.	18 27·3 S.
22	3 45·62 W.	17 29·6 S.	3 42·35 W.	0 13·7 N.	...	...
Means	3 45·49 W.	17 29·8 S.	3 41·85 W.	0 13·0 N.	0 38·48 W.	18 31·1 S.

Date of Comps.	No. 1183 or W. R.A.	No. 1183 or W. Decl.	No. 1203 or Y. R.A.	No. 1203 or Y. Decl.
1868.	m s	′ ″	m s	′ ″
May 19	2 38·31 E.	...	2 58·99 E.	13 43·0 S.
21	2 38·38 E.	11 17·4 S.	2 58·30 E.	13 53·2 S.
	2 37·81 E.	11 10·5 S.	2 59·11 E.	13 50·1 S.
22	...	...	2 59·12 E.	...
Means	2 38·17 E.	11 14·0 S.	2 58·88 E.	13 48·8 S.

The letters N. S. E. and W. denote whether the star is north, south, east, or west of *η Argûs*.

"By two comparisons of No. 387 with 1183, on the 22d May, I found that the former was $3^m\ 45^s{\cdot}50$ *west* and $14'\ 44''{\cdot}5$ *south* of the latter. Adopting $2^m\ 38^s{\cdot}17$ E. and $11'\ 14''{\cdot}0$ S. as the position of No. 1183, with reference to *η Argûs*, we have for the position of No. 387 or Z, with reference to *η Argûs*, as follows:—

R.A. = $1^m\ 7^s{\cdot}33$ *west.* Decl. = $25'\ 58''{\cdot}5$ *south.*

"I have also observed the stars with the transit instrument, but these observations give only the difference of R.A. The co-ordinates of the stars as I have given them agree with your Cape Results, precession &c. considered, within quantities which may be regarded as due to errors of observation rather than to changes in the relative positions of the stars.

"I trust the foregoing results, imperfect as they are, may be of interest to you, and I hardly need say you are at liberty to make what use of them you may think proper. Not being a Member of the Society, I do not receive the *Monthly Notices* as they are issued: hence the lateness of this communication.

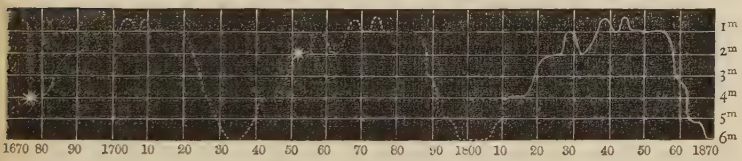
"*Windsor, New South Wales,*
February 9th, 1869."

On the Period of η Argûs. By Prof. Elias Loomis.

In the *Astronomische Nachrichten*, vol. lx. p. 61, Prof. Wolf has endeavoured to show that the observations of *η Argûs* may be represented by a period of forty-six years, with one principal maximum and two secondary maxima in the intensity of its light. The observations recently published by Tebbutt (*Monthly Notices*, vol. xxviii. p. 266) indicate that this period is too short. I find that all the observations of the present century may be represented by supposing the period to be sixty-seven years; and if the curve representing the variations of brightness be twice repeated it will perfectly represent the two observations of 1677 and 1751. Future observations may require us to modify this conclusion; but at present it seems probable that *η Argûs* is a periodic star whose changes of brightness vary from the first to the sixth magnitude, and whose period is about seventy years.

The following table contains all the observations of this star with which I am acquainted, and the accompanying figure shows the curve which represents them.

Date.	Mag.	Authority.	Date.	Mag.	Authority.
1677	4	Halley	1845	1	Jacob
1751	2	Lacaille	1850	1	Gilliss
1811	4	Burchell	1854	1	Tebbutt
1815	4	Burchell	1856	1·5	Powell
1822	2	Fallows	1859	3·1	Powell
1826	2	Brisbane	1861	3·6	Powell
1827	1	Burchell	1862	4·9	Tebbutt
1832	2	Johnson	1863	5·1	Tebbutt
1835	1·4	Herschel	1865	5·2	Tebbutt
1838	0·5	Herschel	1866	5·8	Tebbutt
1842	1	Maclear	1867	6	Tebbutt
1843	0·5	Maclear	1868	6	Tebbutt



If you consider this notice of sufficient importance you are at liberty to communicate it to the Astronomical Society.

Yale College, April 9, 1869.

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*Extract of a Letter from Dr. A Winnecke to E. J. Stone, Esq.**

"I have to announce to you (for communication to the Royal Astronomical Society) the discovery of a Comet, which I observed yesterday night:—

	h m			h m s
April 9	12 24	Karlsruhe M.T.	α Comet	10 32 11
			δ Comet	+ 33° 57′

"This stranger will prove doubtless the expected Comet 1858 II., in the track of which it is situated. If I suppose the identity of the two bodies, the perihelion passage will take place June 25 or 26. It is faint, but large, 6′ perhaps 8′ in diameter.

"*Karlsruhe*, 1869, *April* 10."

* Previously issued as a circular, but inadvertently omitted from the last number of the *Monthly Notices*. Ed.

Observations of Winnecke's Comet. By Hall Wortham, Esq.

1869.	Comet's R.A. h m s	Comet's N.D. ° ′ ″	
April 29	10 3 0	36 32 0	(*a*)
May 4	9 57 0	36 42 0	(*b*)

Telescope employed, an achromatic of 6 inches aperture; power 50.
The above positions are *approximate* only.

(*a*). Comet distinctly seen as a faint nebulous patch of some little size, appearing occasionally to brighten somewhat to a centre, and followed at about its own diameter by a small, but brightish, star below the 9th magnitude. A larger star just beyond the upper part of the field. Telescope set by the Ephemeris by Dr. Winnecke (appearing in No. 1753 of *Astron. Nach.* p. 14) and comet found at once nearly in centre of field of view.

(*b*). Night not very clear, and the comet only just visible.

Royston, Herts, May 4th, 1869.

Observations of the same Comet. By S. J. Perry.

The nights following the 11th and 12th of this month were the only ones clear enough to enable us to observe the Comet which is approaching us at present. On May 11th, $14^h\ 7^m\ 13^s{\cdot}7$ Sid. T., the position was

R.A. $9^h\ 49^m\ 32^s{\cdot}7$ Decl. + 37° 4′ 10″

May 12th, $14^h\ 12^m\ 47^s{\cdot}16$,

R.A. $9^h\ 48^m\ 37^s{\cdot}16$ Decl. + 37° 3′ 45″.

These observations are uncorrected for refraction. If it would be of any use I could give you the positions for future fine nights, but I suppose there are so many good astronomers watching it at present that our observations will be of little use. There seems to be a slight condensation towards the centre, but no decided nucleus. This is all that our object-glass will give. I used a power of about 100, with full aperture 8 in.

I have just laid down a telegraphic wire between our two Observatories, which enables me to give the time and R.A. very correctly.

Stonyhurst College, Blackburn,
May 13, 1869.

Note on a Sun-spot seen March 14, 1869. By John Browning, Esq.

On the above date I was observing the Sun about 9.30 A.M.

Saw a large Sun-spot about 20° N. of the equator, and about 2′ from the E. limb.

Definition being very good I was able to use 12 inches aperture and achromatic power 148 and 208.

To modify the light, I employed a *double*-prism solar eyepiece, and a corrected wedge of neutral tint glass.

The dimensions of the spot I found to be from N. to S. 14,400 miles, from W. to E. 19,600 miles. When comparing these measures with the drawing, it must be recollected that due allowance has been made in this calculation for the great foreshortening which occurs, owing to the spot being so near the edge of the limb.

The umbra contained three nuclei of very unequal dimensions, arranged nearly in the form of an equilateral triangle.

The drawing I made of the spot I have the honour to submit to the Society.*

Two bridges crossed the spot at an angle of about 40°. These bridges presented the appearance of broken twigs lying mostly in the direction of the bridges. It will be seen that both the bridges appear to be fed from a point to the N.E. of the spot.

The edge of the penumbræ furthest from the Sun's limb consisted almost entirely of bright faculæ. The extreme edge of the penumbræ was much darker than other portions, a point lying S.E. being the darkest of all.

This distinct darkening of the extreme edge of the penumbræ has often been doubted and ascribed to the effect of contrast; but if the spot were spirally formed, it would, I think, present some such appearance as I have described.

I have heard from Mr. G. P. Bidder, Jun., that on the afternoon of May 13, he saw a fine large spot, in which the bridges were spirally arranged. This observation I think very interesting.

During moments of the best definition the photosphere resembled broken branches, covered with long leaves, and overlying each other in all directions.

I would wish to speak with all diffidence on such a difficult matter of observation, but I cannot help thinking that the smaller granulations on the photosphere change considerably in appearance. I have seen them in the form of broken twigs, rice grains, and willow leaves, that is, sometimes blunt and sometimes sharp at the extremities, while at other times they were little more elongated than a common egg.

The very careful and interesting series of drawings taken by the Rev. F. Howlett show that Sun-spots have a tendency to assume regular forms. The cloudy matter which forms the photosphere, may frequently assume an appearance similar to that we may occasionally see in our own clouds, sometimes spoken of as "mackerel-back" clouds. If this be so it will be of little use to dispute as to whether these bodies are shaped like rice-grains or willow-leaves. We had better simply note their forms whenever we are so fortunate as to observe them distinctly.

* The drawing was exhibited at the Meeting. Ed.

On a Remarkable Sun-spot observed May 1st, 1869. By G. P. Bidder, Esq.

The accompanying drawing* represents the largest of a very fine group of spots, as it appeared upon the afternoon of Saturday, May 1st. It was chiefly remarkable for a distinctly apparent spiral arrangement, which was very striking. The whole of it gave the impression of an immense whorl, as shown in the drawing, which by no means exaggerates the resemblance.

The spot was traversed by an unusually attenuated bridge, which also appeared to participate in the spiral arrangement. The shortness of time during which the Sun was visible prevented my doing more than sketch the outline of the spot. One measurement was obtained which showed the penumbra to be in length about 27,000 miles. I examined the same spot again on the following day, when all traces of a spiral arrangement had entirely disappeared.

On a proposed Form for Spirit Levels. By Major Tennant.

My late employment has caused my thoughts to revert to some old projects for improving the spirit-level, and the subject is of much importance now that reliance is being daily more and more placed on the indications of these accessories. My original examination was, I think, in 1853, when I entered into an investigation of the properties a spirit-level should have on the supposition that the motion of the bubble was made by bodily transfer.

This hypothesis is widely removed from fact if we look to great movements of the level, but very nearly true for small ones,† and it leads me to a form of level and mode of construction which I have never seen elsewhere, and which I still think would produce results far superior to and far more certain than those of the present process.

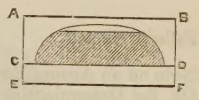
I arrived at the conclusion which will, I think be received, that *the volume or contents of a bubble should be large, that its density relatively to that of the fluid should be small, and that its floor, or the surface common to the fluid and bubble, should have as large an area as possible,* and that these properties would be best gained by increasing the radius of curvature of the upper

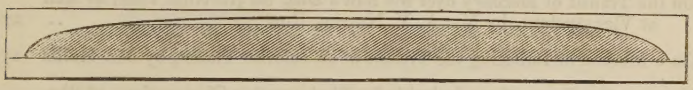
* The drawing referred to was exhibited at the Meeting.

† If we make an alt-azimuth revolve about its vertical axis smartly, we see the bubble pass to the end of the tube, then to the other, and finally settle; and any great and sudden change of level causes a wave to pass along the floor of the bubble till equilibrium is established: but if only a small change be made by the foot-screw the bubble moves sensibly as one mass.

art of the level tube and making the fluid as heavy as possible compared with the bubble. In order that temperature should produce as little effect as possible on the length of the bubble, I would also reduce the volume of fluid, and to gain all these points I proposed to abandon altogether the cylindrical form of level tube and to cut out the necessary cavity from a rectangular prism of glass by means which should ensure that the longitudinal section of the cavity should be a segment of a circle and the transverse one uniform.

I proposed to take a prism, A B C D, of glass and to grind out of it a cavity of a somewhat elliptical shape transversely, as shown in the figure on the margin and in longitudinal section as below. C D E F is a flat plate cemented on to close the level.





Instead of filling the cavity with a volatile fluid of small density, whose vapour mixed with air forms the bubble, I think that the strength this new form might have would allow the use of mercury for the fluid and hydrogen perhaps (it would be necessary to exclude air) for the bubble. If mercury could be used it might be possible to float on it a plate of glass or iron carrying a mark, whose position could be referred to a scale with far more accuracy than the ends of the bubble can now be read; but I fear that this would be quite impracticable, and that the plate would have to be rejected from its liability to stick, if it fitted to the shape of the bubble floor closely enough to be useful.

At the time I have mentioned, I proposed, in a letter to the Surveyor General, to try making a level of this sort but difficulties intervened of various sorts, and it was laid aside. Any one probably would be able to devise the necessary apparatus for cutting the glass truly, but what I proposed was to have a cutting wheel revolving rapidly about a vertical axis and to traverse the glass plate before it. If it were rigidly fixed to two straight edges guided by cylindrical studs on a table, the longitudinal section would be a circle, and the form of the final cutting wheel would define the cross section.

It is hardly possible that I shall now be able to try this project (which I had nearly forgotten), but I have mentioned it to several persons, and I now offer it to the Astronomical Society in the hope that it may lead to an improvement in the mode of constructing levels.

Calcutta, September 25th, 1868.

ERRATUM.

In Vol. XIX. of the Memoirs of the Royal Astronomical Society.

Page 234, line 13 from the top,—

for $B = \varrho_2 \cos \phi_2 - \varrho_1 \cos \phi_1$, *read* $B = \varrho_2 \sin \phi_2 - \varrho_1 \sin \phi_1$.

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